# AMPERE AND THE PROGRAMMING OF RESEARCH

*By Christine Blondel**

In the evening of 25 September 1820, the day he had read his first memoir on electrodynamics at the Paris Academy of Sciences, Ampère wrote to his son that "the crucial experiment that [he] had conceived of as a definite proof" of his theory only succeeded that very morning.[1] But he did not make clear which experiment.

In his review of my book on Ampère's electrodynamics, L. Pearce Williams records his belief that my reconstruction of Ampère's early researches is wrong, and he refers to his own article "What were Ampère's Earliest Discoveries in Electrodynamics?"[2] In fact the only differences I found between the account appearing in his article and mine are as follows.

* Centre de Recherche en Histoire des Sciences et des Techniques, Musée de La Villette, 211 Avenue Jean Juares, 75019 Paris.

[1] *Correspondance du grand Ampère*, ed. L. de Launay, 3 vols. (Paris: Gauthier-Villars, 1936–1943), Vol. II, p. 562.

[2] Christine Blondel, *Ampère et la création de l'électrodynamique* (Paris, Documentation Française, 1982); rev. by L. Pearce Williams, *Isis*, 1984, *75*:592; Williams, "What Were Ampère's Earliest Discoveries in Electrodynamics?" *Isis*, 1983, *74*:492–508. See also Blondel, "André-Marie Ampère: La physique newtonienne face à l'électromagnétisme," (Diss., Centre A. Koyré, Ecole des Hautes Etudes en Sciences Sociales, Paris, 1978).

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***Concerning the magnetic action of a pile and the origin of electric currents in magnets.*** This action of the pile would have permitted Ampère to attack Biot's electrostatic theory of the pile. But Ampère did not pursue this critique: the pile's action served primarily to justify the existence of electric currents in the earth. In fact, no document whatsoever exists that presents this action as the source for the idea of the existence of currents in magnets. Ampère always recognized the fundamental role of the intermediate hypothesis about currents within the terrestrial globe. Moreover, the model of a circular pile (which he very likely never achieved) follows from the explanation of magnetism by electric currents.

***Concerning public disclosure (18 September).*** According to L. Pearce Williams, Ampère "did not convey that idea [of the nature of magnetism as electricity in motion] to anyone" by 18 September.[3] But I would argue that he did publicly announce it before the Academy on that date. Moreover, he used it to make a prediction! The text Ampère read to the Academy, which accords with his later claim to have predicted the results of his experiments on helices and spirals, states: "M. Ampère *annonce* que d'après sa théorie [des hélices] *doivent*, quand des courants les traversent, exercer une action pareille à celle des barreaux aimantés."[4]

This agrees entirely with the abstracts published in the *Journal de Physique* only a few weeks later. Indeed, Ampère could not have altered his spoken words for publication; for one thing, his colleagues would not have forgotten what he had said just a few sessions earlier. Even though the original presentation of the idea was rapid and a bit muddled, we may reasonably think that it did enter the public domain on 18 September 1820.

***Concerning the "crucial experiment" (25 September).*** In Williams's view, two experiments are involved, not one (which does not agree with Ampère's use of the singular form): the action between two plane spirals and the action between two helices. The first one would have succeeded; the second one, meanwhile, would have produced an anomalous result. Ampère would consequently, according to Williams, have dropped all mention of helices at the 25 September afternoon session. But Ampère could not have observed the anomalous effect shown by two parallel helices when using his first set-up. His original helices were movable around their middles, so that the only motion they could undergo was rotation. The anomalous effect was only discovered later, using another instrument allowing translational motion.[5]

Ampère probably experimented, as he announced, with helices and verified with some difficulty that they acted like magnets when laid end to end. But he chose to show his Academy colleagues only the easier, much more convincing experiment with spirals. This one was his so-called "crucial experiment."

***Concerning chance discovery and the action between two parallel currents.*** In my view, therefore, the anomalous effect between helices could not play any role in passing from parallel spiral currents to parallel rectilinear currents. Nor did the magnetization of a steel needle inside a helix, which was achieved later, play a role.

Williams suggests that Ampère would have not failed to observe the attraction between two parallel wires drawing the current in a spiral. It was entirely possible for him to have observed this effect. But if Ampère did observe it, it must have been extremely weak and uncertain, because he did not speak about it at all at the next Academy session. Yet this was the phenomenon of fundamental importance for him. He would not have deliberately concealed it had he believed his experiment to have demonstrated its existence conclusively.

In fact, the action between parallel cur-

[3] Williams, "Ampère's Earliest Discoveries," p. 502.

[4] "Analyse des Mémoires lus par M. Ampère à l'Académie des Sciences, dans les séances des 18 et 25 septembre, des 9 et 30 octobre 1820," *Annales générales des sciences physiques*, 1820, *6*:240 (emphasis added).

[5] This point is not detailed in my thesis but can be gathered from an Ampère manuscript I have published (Christine Blondel, "Sur les premières recherches de formule électrodynamique par Ampère," *Revue d'histoire des sciences*, 1978, *31*:53–65, on p. 65) and from Ampère's memoir in *Annales de physique et de chimie*, 1820, *15*:173.

rents was already programmed by this time, and what he had yet to achieve was a real experiment "to prove" it to the Academy.

As a final remark I would like to emphasize the importance of the idea of the programming of research. Ampère's program allowed alternative paths at various junctures and therefore made room for the accidental. Serendipitous discoveries could be incorporated within the program, as one of the sources of alternative paths.

***Reply*:**

The following addresses the above points in order.

***Magnetic action.*** The circular pile follows *directly* from Ampère's mapping of the galvanic circuit. It makes more sense to go from this to currents in the earth, than the other way around.

***Public disclosure.*** The "Analyse" states that Ampère mentioned helices only after having presented his theory in "le paragraphe suivant." This paragraph, as a note on page 241 clearly states, was *not* read on 18 September, but deferred until 25 September. No other source mentions spirals or helices except Ampère, two months later, by which time his colleagues probably were not as clear as Blondel assumes on what he had said at each meeting of the Academy.[6]

***Crucial experiment.*** The crucial experiment was to show that magnets were nothing but the results of electricity in circular motion. To do this, Ampère had to show that two helices acted just like magnets, for the action of a magnet on a helix proved no more than did the action of a magnet on an iron nail (a point Ampère later made). Hence his need for two voltaic piles (he thought), and hence the experiment as I have described it.[7] Also hence, his puzzlement over the results of this experiment, the abandonment of the helices, and the substitution of the spirals, which worked. The helices were restricted to rotation only *after* Ampère discovered why his earlier experiment failed. It was the compensatory wire run down the middle of each to destroy the longitudinal effects of the currents that made them capable only of rotation. Blondel simply ignores the sources here.

***Chance discovery.*** I did not merely suggest that Ampère might have accidentally seen the interaction of two rectilinear currents: I cited a source. Blondel again ignores it.

***Ampère's program.*** What program? Between 18 September and 9 October, Ampère sought only to show that magnets and circular currents were equivalent. That's hardly anything so philosophically fancy as a program.

I conclude by restating what I said in my review. Blondel's book is the best there is on Ampère's work on electrodynamics.

L. PEARCE WILLIAMS
*Department of History*
*Cornell University*
*Ithaca, New York 14853*

[6] All documented in Williams, "Ampère's Earliest Discoveries" (cit. Blondel n. 2), esp. pp. 497–498, 502; "Analyse" (cit. Blondel n. 4).

[7] *Ibid.*, p. 504.